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Response to Referees Criticisms of "Generalization of the Greenberger-Horne-Zeilinger Algebraic Proof of Nonlocality" by Robert K. Clifton, Michael L. G. Redhead, and Jeremy N. Butterfield

Dear Prof. van der Merwe:

Thank you for your letter of June 27/90. I am sorry it has taken us so long to respond to your referees criticisms, but I had left for Canada for a month's vacation by the time your letter and FAX had arrived, and so have only recently been able to organize a meeting with Prof. Redhead and Dr. Butterfield to discuss the criticisms. Unfortunately this also means that it was not possible to respond to you by July 7, as you requested, but hopefully this did not, in the end, cause you too much inconvenience.

In what follows, we shall respond to the points raised by your two referees and suggest changes that we can make in our paper. (We have numbered the sections below for easy reference, if necessary, in subsequent correspondence.)

We have not yet enclosed a revised manuscript incorporating such changes because we thought it best to first seek some sort of agreement with your referees over some of the issues they have raised; and also thought we should await any decisions you may make regarding the suitability of our paper for publication. (Incidentally, if our paper is accepted in the end, we hope our delay in responding to your referees has not jeopardized the possibility of our paper appearing in the forthcoming 'Bell issue' of *Foundations of Physics*.)

I. Lightening the Exposition

(1) Both of your referees in their opening sentences say that our exposition could be simplified at points. Specifically, referee 2 (pg. 1) suggests, first, that we minimize our use of acronyms, and that we collect at the end of the paper for easy reference those we do use, noting where they were first introduced.

We agree with this, and propose to drop at least the acronyms 'TF'and 'OF_0' because they are used at only a few spots, and the acronym BKS because it is only used in a section which we now wish to delete (more on this later in paragraph V(13)). This brings the acronym total down to 13, most of which are physical locality principles referred to many times, and hence indispensible if our presentation is to be unrepetitive. For these, we shall create an appendix/glossary in which each is briefly defined in nontechnical terms, and the page number of its exact definition given.

(2) Referee 2 (pg. 1) also suggests that we number each equation, presumably because although we may not refer back to an equation, the reader or future writers on the GHZ proof may wish to, and numbers facilitate such easy reference.

We agree with this, and shall number all equations when revising.

(3) We agree with referee 2 that we can simplify our sections 3 and 7 proofs by using *finite* subsets of $\Theta_{\pm}$ rather than *countable* subsets, even though the proof is obviously valid for both cases. So we shall drop this "heavy machinery". But we prefer not to specialize initially to our *five* particular setting choices, until we've proved that the broad class of local stochastic theories under consideration 'collapses' to a deterministic class. For specializing too early would surely leave the reader baffled until the final stage of the proof about the reason for our setting choices. In this we are following GHZ, who don't reveal how they propose to prove a contradiction, until after they've informally run the EPR argument to justify their deterministic hidden-variables framework. (Incidentally, our initial reason for using countable subsets was to provide a valid proof as close as possible to our interpretation of "continuously", in the published GHZ proof-sketch, as "like the continuum". More on this proof-sketch below.)

II. The Published GHZ Paper - Misinterpreted?

(4) Again, both your referees seemed to say that we have seriously misinterpreted the proof-sketch published by GHZ. Let us begin with the specific comments made in the shorter1-page referee report by referee 1. S/he claims that the published GHZ paper in fact contains no flaw, because it never gives a proof.

We agree that the *details* of the proof were not given; however the *two main stages* of the proof were in fact spelt out quite explicitly on the final page of GHZ's paper where they say:
"The condition corresponding to the super-classical case is

$$A_\lambda(\alpha)B_\lambda(\beta)C_\lambda(\gamma)D_\lambda(\delta) = \pm 1$$

$$\text{for } \alpha + \beta - \gamma - \delta = 0,\pi$$

But it turns out that there is no way to satisfy this condition. It is too restrictive, because we can continuously vary two of the parameters while keeping the other two constant. This leads to the conclusion that A=B=C=D=constant. But this is impossible, since the product sometimes equals +1 and sometimes equals -1".

We deliberately quoted the relevant part of this passage on pg. 14 of our own paper, in order to substantiate our interpretation of their suggested proof. And we *continue* to maintain that this passage makes the two main steps in their proof-sketch quite clear: first, use the above mathematical condition to prove each response function is constant for all values of its argument (e.g. $A_\lambda(\alpha)$ = constant, for all $\alpha \in [0,2\pi)$, etc.); second, note that the product $A_\lambda(\alpha)B_\lambda(\beta)C_\lambda(\gamma)D_\lambda(\delta)$ must be a constant for all $\alpha,\beta,\gamma,\delta$, contradicting the displayed condition. Furthermore, this proof-sketch is internally coherent, despite its shaky foundations (more on this below in III(7)). And after we submitted our paper in April, one of us obtained (from Prof. N. David Mermin) a copy of Prof. Greenberger's lecture notes, which presumably existed prior to the GHZ paper, in which the GHZ proof we had reconstructed for ourselves appears explicitly and in detail in both its stages - thus confirming our interpretation of their proof-sketch (cf. equations (18) - (28) in Section VIII. 'Going Beyond Bell's Theorem --1. Efficient Counters', D. Greenberger, unpublished lecture notes). To sum up, our interpretation of the GHZ proof-sketch as published is not faulty, as both your referees seem to think.

(5) Further to this point, referee 2 (pg. 2, paragraph 1) holds that we were well aware of the structure of the *real* proof that GHZ subscribed to,

since two of us attended a conference (in Sicily) in which that proof was given.

We reply: in fact only one of us (Clifton) actually attended the Greenberger lecture in Sicily (Prof. Redhead had left by then), and I (Clifton) recall vividly that Greenberger said no more about how to prove his theorem than he says in the published GHZ paper. I was puzzled about the actual details of the proof. And, indeed this was the very reason that I embarked on attempting to verify the proof-sketch's first stage. And it appears I was not alone in being puzzled - Prof. N. David Mermin (who also attended) was given the same impression. Indeed, Mermin seems to see the distinction between what Greenberger said in Sicily, and the new proof we give in our paper. He writes about our paper, of which we sent him a draft in February: "After a bit I realized that your essential contribution to the argument was getting the contradiction out of measurements at a finite (indeed small) number of detector settings" (private communication, March 23/90).

We don't mean to belabour this point. But we do want to reassure the referees that we have always intended to abide by "the ethics of professional reading" as outlined by referee 2 (pg. 2) - with which we wholly agree.

However, we do agree that some toning down of our wording at various places is called for in light of this situation; we give details in IV.

III. The Alternative GHZ Proof, in Greenberger's Lecture Notes, Referee 1's Report, and Forthcoming GHZ/Shimony Paper - Is it Valid?

(6) Since submitting our paper, we have realized that GHZ have an unpublished proof that is an alternative to the original, the original being spelt out in Greenberger's lecture notes - cf. II(4). The difference lies in the second stage. While the original works with A,B,C,D = constant, the alternative exhibits a contradiction directly in terms of one of the response functions, e.g. A (cf. e.g. the cited contradiction between eqs. (28) and (30) in Greenberger's lecture notes, which appears as an afterthought to his detailed presentation of the original proof).

We have three sources for this alternative: the lecture notes, referee 1's report, and a new unpublished paper by GHZ and Shimony entitled "Bell's Theorem without Inequalities", kindly sent to us a few weeks ago by Shimony.

We have two main comments to make about this alternative, one here, and one in (7) below. First, our own proof is similar: it also works with a contradiction directly in terms of one of the response functions. But surely we cannot be expected to have anticipated their having this alternative.

(7) Similar, but not the same! The alternative GHZ proof *still* fails to circumvent the objection we make to their original proof. Thus, referee 1 asserts that it can be proved for four sets of angles (that satisfy the four equations s/he gives) that $A_\lambda(2\theta) = A_\lambda(0)$, which s/he claims is then true "for any angle".

But in the measure-theoretic framework we adopt in our paper, this is not so. For any angle θ, each of the four equations s/he cites is true for *almost all* states λ; and together they imply that *for any angle* θ, $A_\lambda(2\theta) = A_\lambda(0)$ is true for *almost all* states λ. But from this it does not follow that *there exists* a state λ such that *for any angle* θ $A_\lambda(2\theta) = A_\lambda(0)$ is true; unless the set of angles at issue is countable!

To put it in logical terms: one can justify reversing the order of the two relevant quantifiers, only if one incorrectly assumes that the measure of the intersection of an uncountable infinity of measure-1 sets must be non-empty. We point this out explicitly on page 14 of our paper in relation to the same mistake in the original GHZ proof (which *also* claims to arrive at the conclusion that $A_\lambda(\alpha) =$ constant, *for all* α).

Therefore the main difference between us and GHZ is that in our proof we never claim that any of the response functions A-D can be proved constant *for any angle* , and indeed our contradiction does not rely on such constancy!

(8) Incidentally, referee 1's third and final paragraphs claim that GHZ's alternative proof, in not restricting itself to 5 sets of angles, is "more general" than ours because they show that there is more than just a measure-zero set of contradictions.

We disagree: on pg. 13, para. 2 of our paper we point out that our proof allows many contradictions to be derived because of the "uncountably many different ways to choose the setting quadruple $(\theta_A,\theta_B,\theta_C,\theta_D)$" defining the 5 sets of angles we choose. And we say in the next paragraph that the proof thus applies for uncountably many setting choices. (In this, referee 2 (p. 1, para. 2, line 13) sees it as we do!)

IV. Proposed Compromises over the Central Issues

(9) In light of the above, we feel it is unreasonable to expect us to change our paper so that it credits GHZ (or GHZ/Shimony) with obtaining the first measure-theoretically valid proof of contradiction which uses five sets of angles, as our proof does. Having made our position as clear as possible, we would nevertheless like to propose some compromises.

First, we are of course delighted to credit GHZ, not just with the innovation of their published paper, but also with having a detailed proof similar to ours.

(10) Secondly, to be fair to GHZ, we must admit that they formulated their proof without regard to a measure-theoretic framework, and to that extent were entitled to assume that probability 1 = certainty. And there are good precedents for such an assumption (e.g. EPR themselves!). Because of this subtlety, we propose to speak throughout our text of our proof as "refining" GHZ's proof of contradiction (the term Mermin uses in his recently published Physics Today article), rather than "correcting" it. This does not mean, however, that we propose to back away from our interpretation of GHZ's originally published proof-sketch.

(11) In the spirit of the above 'compromises', we propose to preface our proof with a few sentences like the following (to be inserted between the first two paragraphs of section **3** in our paper):

This is not the form in which GHZ state their theorem. They do not use measures; since they launch their argument from EPR's premises which equate measure 1 with certainty, specifically EPR's sufficient condition....(there follows our interpretation of GHZ's proof-sketch)...It effectively assumes that the intersection of an uncountable infinity of measure-1 sets must be non-empty. Fortunately, alternative derivations can be given. Greenberger *et al.* themselves have one (ref. to unpublished GHZ/Shimony paper), though on account of their own non-measure-theoretic framework, they do not articulate the above flaw. In any case, our alternative derivation (discovered independently of theirs) follows.

V. Substantive But Less Central Criticisms from Referee 2

(12) We agree with referee 2 (pgs. 2 - 3) that in using TF and AI, not Shimony's weaker outcome independence and parameter independence, we are not generalizing GHZ as much as we might.

But we have three points in reply to this: (a) As so often, there is a trade-off between using premisses as weak as possible for the task at hand, and using premisses for which a simple physical motivation can be found. Being a trade-off, it's a matter of judgment: ours is that TF and AI are the best assumptions to use because they are motivated solely by 'locality'. (b) Another point in favour of using TF and AI is that (as the referee 2 (p. 3) agrees), since they imply Shimony's assumptions, they shed new light upon the consequences of denying Shimony's (which have been surprisingly little discussed in the literature - nor even distinguished from Jarrett's TF assumption, as they should be!). (c) We can and do still maintain that, overall, our introduction of a "stochastic hidden-variables" framework generalizes the "deterministic hidden-variables framework" used by GHZ in their paper. We also generalize their proof by providing (cf. our section **7**) a version which does not rely on strict correlations (a point which referee 2 does not mention).

(13) We agree with referee 2 (pg. 3, para. 2) that our claim in section **5** about the Gleason theorem is wrong, although we are not exactly sure what her/his reasons are. In any case, we now see that a suitable choice of a countable set of triads (e.g. all triads at a rational angle from a given triad) allows a Gleason-style proof of no-hidden-variables to proceed with our 'Weakened Value Rule'.

We would like to thank referee 2 for prompting us to see this. We propose to drop section **5** of the paper, which is mainly concerned with elaborating that claim. (This also means dropping p. 15's reference to colouring the sphere; however we do agree with referee 2 (pg. 3) that Belinfante's work should have been cited.)

(14) As to referee 2's point about measures (pg. 3, para. 3), we agree that in general densities may not be defined. But altering our notation to account for this would require unwieldy subscripts to distinguish our different measures, thus making our paper more difficult to read. Furthermore, the density notation is more familiar to readers of the Bell literature (e.g. Bell's own book uses it).

So we propose to add a footnote upon first introducing densities, to the effect that: "Our notation using densities assumes absolute continuity of our probabilities relative to the postulated measures on M and Λ. But this makes for a familiar notation, especially with regard to the Bell literature; and in any case, all our arguments can be generalized in this respect".

(15) Finally, referee 2 (pg. 4) says Kochen's ideas pre-date Heywood and Redhead's proof. We would point out that credit has been given in the appropriate place. On the first page of the Heywood-Redhead paper a footnote mentions that A. Shimony had pointed out that a related unpublished idea of S. Kochen's exists.

In any case, we've proposed in (13) to omit the entire section containing the reference to Heywood-Redhead, so the question of priority no longer arises.

We hope this letter clarifies our position to your referees. Please also pass on our sincere thanks to them: their comments have been very helpful in improving our paper. We look forward to hearing from you regarding the status of our paper in due course.

Yours sincerely,

Robert K. Clifton
(written by Clifton and Butterfield
after discussion with Redhead)